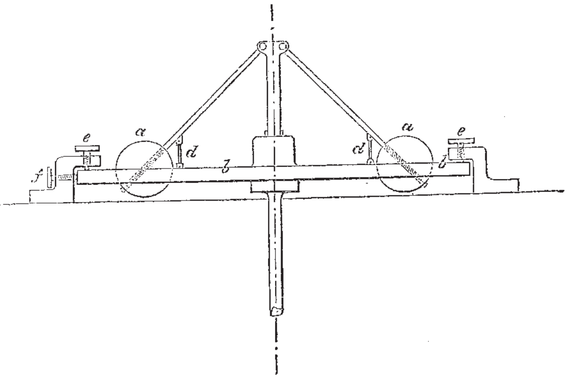
in use on the clock of the apparatus of Cape Bon, Tunis, an apparatus exactly similar to that now standing in the International Exhibition. It consists of a shaft making 170 revolutions per minute, to which the balls *a a* are hung, and on which the disc *b b* can slide, guided by a feather key. When the clock is below speed the disc rests upon a collar fixed on the shaft, the pull exerted by the balls through the links *d d* being insufficient to raise it; but as soon as the proper speed is attained, the disc rises and comes in contact with the screws *e e*, which are tipped with leather and fixed to the frame of the clock. Spaces are cut out of the disc to admit the balls, avoiding unnecessary height. The screw *f* serves as a brake to stop the clock at pleasure. I



calculate that work to the extent of five foot-pounds per minute must be done on the governor to accelerate the clock one second per hour. This form possesses two advantages over that in which the rubbers are carried by the balls—1. It checks any acceleration of the clock more powerfully; 2. It is easier to adjust. In the older form it is necessary to ascertain by careful experiment that *each* ball shall bring its rubber into contact exactly when the speed is correct, whereas in this it is immaterial that the arms of the balls should be exactly equal; it is only needful that they should *together* raise the disc to contact when the speed is right. J. HOPKINSON

Glass Works, near Birmingham, Sept. 1

Rainbows

As a pendant to my note inserted in NATURE, vol. x. p. 437, I may mention that an exceedingly fine lunar rainbow was observed here at 8.40 P.M. on September 29.

Though the moon was near the last quarter, the bow was bright enough to appear reddish on one side and greenish on the other. It is the only one, of some five or six lunar rainbows I have seen, which appeared to show any trace of differences of colour.

I may also mention that about the end of August I saw, two hours after sunrise, a dazzlingly bright and gorgeously coloured parhelion in a small ice-cloud to the right of the sun, the rest of the sky being almost perfectly clear. There had been a sudden and considerable fall of temperature during the previous night.

St. Andrew's, Oct. 2 P. G. TAIT

IN NATURE, vol. x. p. 438, Mr. Schuster complains that in text-books no mention is made of supernumerary rainbows, and that the theory of them is to be sought in original memoirs, not generally accessible. Allow me to mention that in Sir John Herschel's Meteorology (a little work published by Black, price three and sixpence, and originally an article in the Encycl. Britann.), a complete explanation of the rainbow, and of the supernumerary bows as well, on the principle of interference, is to be found. F.M.S.

U.S. Weather Maps

IN Prof. Loomis's "Results of an Examination of the U.S. Weather Maps for 1872 and 1873" (published in the *American Journal of Science and Arts*, and recently noticed in NATURE, I am struck not only by the general agreement but by the almost verbal coincidence of one or two of his "Results" with some of the rules laid down in my work on the "Laws of the Winds prevailing in Western Europe," which was published in the beginning of 1872.

In "Laws of the Winds," Part I. p. 56 and following, I have shown that "we are unable to account for the eastward progress of depressions by attributing it to prevailing westerly upper-currents," but that "each system of depression appears to travel eastward with a kind of self-developed motion," and that the precipitation on the east side of the centre "is the principal agent in producing the change of geographical position." Prof. Loomis writes: "The progress of a storm eastward is not wholly due to a drifting resulting from the influence of an upper-current from the west, but the storm works its way eastward in consequence of the greater precipitation on the eastern side of the storm."

Prof. Loomis also appears to attribute the formation of some depressions, primarily developed in the United States, to the collision of moist air from the Pacific with the mountains in the north and west, in the same way as I have attributed the primary formation of some of our depressions to the collision of the vapour-laden atmosphere from the Atlantic with the high-lands in the west and north of the British Isles.

I am glad to observe that Prof. Loomis is no advocate of the "circular theory" of storms as still held by some meteorologists. He intimates the mean inclination of the wind towards the lower isobars as "more than 45°" in the United States. In the Journal of the Scottish Meteorological Society, No. xxxix. I have shown that at stations in the British Isles the mean inclination is 21°, but that it appears to be considerably higher in continental Europe.

In the work previously alluded to I have shown that depressions appear to travel most to the south when the atmosphere is warmer in the west than in the east, and most to the north under contrary circumstances, but that this influence is interfered with by another, viz., the tendency of depressions to travel so as to have the highest general pressures on their right. A less limited acquaintance even than I can claim with the U.S. Weather Maps would go far to show which of these two influences is the predominant, the general atmospheric conditions of the United States presenting a better field for their investigation than is to be obtained in Europe. Prof. Loomis finds that in North America storms tend most to the south in July and to the north in October. It would be interesting to inquire whether this observation holds good of depressions on the Pacific coast, as well as near the Atlantic. But a two years' average is insufficient to settle such questions.

On the whole it is satisfactory to find that some important results obtained from a study of European weather-charts are found, on good authority, to be in accordance with those derived from the U.S. maps. At the same time some of the theoretical remarks made by Prof. Loomis will not, I think, be generally endorsed by meteorologists. The statement that "it needs no argument to prove that when the wind is flowing from all quarters inwards upon a central area, there is a rapid accumulation of air, which can only escape by an upward motion," is incorrect; the depression of the barometer in the centre showing that there is no accumulation, but a rarefaction, produced in part, as Prof. Loomis has himself previously shown, by precipitation, and which is itself the cause of the influx.

Under the present conditions of anemometry all endeavours to calculate the upward movement in a storm from anemometrical data should also be accepted with much reserve. Still more hazardous (considering the inclination of depression-axes and the frequent difference of direction between currents at small and those at great elevations) is the attempt, in such an inquiry, to correct the observed velocities at sea-level by those on the summit of Mount Washington. With a depression in Eastern Canada a west wind not uncommonly blows on Mount Washington while more southerly airs are felt at the three nearest stations. If in such a case we calculate the amount of influx towards the depression-centre simply from the ratio between the velocity at sea-level and that on Mount Washington, it is obvious that the result will be the reverse of accurate.

Aug. 25 W. CLEMENT LEY

Aurora

ON Sept. 11 I was at Kyle Akin (Skye). The day had been wet and stormy, but towards evening the wind fell and the sky became clear. About 10 P.M. my attention was drawn to a beautiful auroral display. No crimson or rose tint was to be seen, but a long low-lying arc of the purest white light wa

formed in the north, and continued to shine with more or less brilliancy for some time. The arc appeared to be a double one, by the presence of a dark band running longitudinally through it. Occasional streamers of equally pure white light ran upwards from either end of the bow. The moon was only a day old, but the old landscape was lighted up as if by the full moon; and the effect of Kyle Akin lighthouse, the numerous surrounding islands, and the still sea between, was a true thing of beauty, forming as it did a quiet contrast to the more brilliant but restless forms of auroræ generally seen. I particularly noticed a somewhat misty and foggy look about the brilliant arc, giving it almost a solid appearance. The space of sky between the horizon and the lower edge of the arc was of a deep indigo colour, probably the effect of contrast.

I regretted I had no spectroscope with me, as it would have been a fine opportunity to test the spectrum of an aurora of pure white light. I had a strong impression that the bow was near to the earth, and almost thought that the eastern end, and some fleecy clouds in which it was involved, were between myself and the peaks of some distant mountains. The eye is, however, deceptive in such cases, though instances are not wanting of auroræ close to the earth's surface. I shall be glad to know if other observations of this aurora were made.

Nairn, N.B., Oct. 3 J. RAND CAPRON

The Cry of the Frog

THE fact that the common frog (*Rana temporaria*) is capable of crying out lustily when he feels himself in danger, does not seem to have been frequently remarked. In my small walled garden there is a common frog who is persecuted by three cats. His residence is a heap of slates at the foot of an ivied wall, and here he is safe. But if he ventures far abroad his tormentors soon espy him, and though they seem nearly as much terrified as himself, they cannot resist the temptation to touch him with their paws. He immediately opens his mouth and utters a prolonged cry, which appears to be very surprising to the cats, who draw back for a few moments, and then pat him again, apparently out of mere curiosity, to be again scared by the same unusual sound. This sound is a shrill and rather sibilant wail, like the note of a small penny trumpet or the cry of a new-born infant. There can be no mistake about it, as I have repeatedly touched the frog with my own hand after driving the cats away, and the same cry has immediately followed, the lower jaw being dropped so that the mouth stands open about a quarter of an inch at the tip.

Leicester, Sept. 26 F. T. MOTT

The Woolwich Aëronautical Experiment

II.

IN order to discover the laws of the vertical motion, we must suppose that the balloon is resting in perfect equilibrium when on land; which means that the ascending power of the gas enclosed in the balloon is just equal to the weight of the canvas, netting, grapnel, ballast, passengers, &c. Under these circumstances the balloon will not ascend by itself, but it will with all the weight of the sand which may be thrown overboard, if a certain space is left for dilatation and the balloon is not quite full when resting on land. If the volume is V at the surface of earth, it will be $\frac{VH}{h}$ at an altitude where barometric pressure is h, being H at departure. When the balloon is quite full, gas escapes by the lower part under the shape of a whitish steam. If v is the additional volume which can be filled by dilatation, that phenomenon will take place at an altitude where the pressure is h given by the equation $\frac{VH}{V+v}$.

We suppose that the height h is never to be attained, and in fact it is desirable for the aëronauts to limit their altitude before starting, and not to fill their balloon with a gas which they are obliged to throw away by the valve or to see escaping by the *appendice* at some risk of their own safety; one of the greatest advantages of the vertical fan being to limit at will the ascent, as will be shown.

In our calculations we suppose that the canvas is not losing gas, that the sun is not affecting the balloon, and that no water is falling upon it, or no cloud concealing the sun. All these changes of temperature can be made the subject of special calculations, and the real motion of the aërostatic globe is the *mean* between all the different circumstances of the atmosphere.

If a balloon starts in an homogeneous air because a weight p of sand was thrown overboard, P being the weight of the air displaced by the balloon when resting on land, the motive power is $g' = \frac{g\,p}{P+p}$ and the laws of the motions of an Attwood machine are perfectly applicable to it.

The elevation takes place with an increased velocity up to the moment where the resistance of the air is = to g'. Consequently,

$$Kv^2 = \frac{p\,g}{P+p}.$$

K being a certain coefficient which depends on the form of the balloon, its diameter, its netting, and the density of the air. K diminishes as the altitude increases, but the diameter of the balloon enlarges gradually to its utmost. As the law of diminution of pressure is not known, we are obliged to suppose K = constant.

If we suppose a balloon of 60,000 cubic feet holding 50,000 cubic feet of gas when resting on the ground, the balloon can reach without losing gas (except by the loss through the canvas, which we suppose to be perfectly gas-tight) to a level where $h' = \frac{5h}{6}$ = about 6,000 feet in round numbers. Under these circumstances the weight of the balloon when resting on land may be supposed to be about 3,300 pounds.

If we suppose 20 lbs. of sand are thrown overboard in ascending, the motive power will be $\frac{g}{115}$. The uniform motion will be $Kv^2 = \frac{g}{115}$.

Under these circumstances, as far as my knowledge goes, it is 4 ft. per second. If we suppose $g = 32$ feet.

$$Kv^2 = 16\,K = \frac{32}{115} \text{ and } K = \frac{32}{115 \times 16} = \frac{2}{115}$$

If a static effort of 20 lbs. in the vertical direction can be produced by the working of the vertical fan, it is easy to understand that the ascent can be stopped before the balloon has reached the level where the gas is beginning to escape by working in the proper direction for it. That effort is not too much for two men working on a fan which is suitably constructed.

The same thing can be said as to the descent of the balloon, but K is much larger, as the shape of the lower part is not so well suited for moving in the air as the upper half. With *appendice*, netting, ropes, and car, it exerts a resistance which is much larger and may be compared with the force exerted by a *parachute* descending in the air. The difference is very great, as I observed several times in my ascents that it was difficult to give the balloon a descending impulsion towards the land. I should not wonder if it was partly the cause of the resistance felt by Mr. Bowdler when moving his fan in the direction where it ought to have caused the balloon to descend; at least such is the opinion that I am in position to hold from the concise and imperfect narrative I found in the public papers.

W. DE FONVIELLE

Is the Rabbit Indigenous?

WOULD you permit me, through the medium of NATURE, to ask on what grounds the rabbit is considered not indigenous in this country? The best authorities on British and German Mammalia seem agreed that it is a native of the Mediterranean basin. On what facts or writings is this opinion based, and at what time was it introduced into Great Britain? I am very anxious to determine whether the above statements are founded on authentic documents or writings, or are merely suppositions which cannot be asserted with certainty. N.

Sept. 30

THE SOCIAL SCIENCE CONGRESS

THE friends of social science have had a most successful meeting this year at Glasgow, and in the various addresses and papers there has been afforded ample evidence that the importance of the introduction of more scientific knowledge into the heads and daily life of the people is becoming more and more widely acknowledged.

In the Health Section, Dr. Lyon Playfair in his address,